

XXII. *Experiments made to determine the positive and relative Quantities of Moisture absorbed from the Atmosphere by various Substances, under similar Circumstances.* By Sir Benjamin Thompson, Knt. F. R. S.; communicated by Charles Blagden, M. D. Sec. R. S.

Read March 22, 1787.

BEING engaged in a course of experiments, upon the conducting powers of various bodies with respect to heat, and particularly of such substances as are commonly made use of for cloathing, in order to see if I could discover any relation between the conducting powers of those substances, and their power of absorbing moisture from the atmosphere, I made the following experiments.

Having provided a quantity of each of the under mentioned substances, in a state of the most perfect cleanness and purity, I exposed them, spread out upon clean China-plates, twenty-four hours in the dry air of a very warm room (which had been heated every day for several months by a German stove), the last six hours the heat being kept up to 85° of FAHRENHEIT's thermometer; after which I entered the room with a very accurate balance, and weighed equal quantities of these various substances, as expressed in the following table.

This being done, and each substance being equally spread out upon a very clean China plate, they were removed into a very large uninhabited room upon the second floor, where they
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were exposed 48 hours, upon a table placed in the middle of the room, the air of the room being at the temperature of 45° F.; after which they were carefully weighed (in the room) and were found to weigh as under mentioned.

They were then removed into a very damp cellar, and placed upon a table, in the middle of a vault, where the air, which appeared by the hygrometer to be completely saturated with moisture, was at the temperature of 45° F.; and in this situation they were suffered to remain three days and three nights, the vault being hung round, during all this time, with wet linen cloths, to render the air as damp as possible, and the door of the vault being shut.

At the end of the three days I entered the vault, with the balance, and weighed the various substances upon the spot, when they were found to weigh as is expressed in the third column of the following table.

The various substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold, uninha- bited room.	Weight after be- ing exposed 72 hours in a damp cellar.	
	Pts.	Pts.	Pts.	
Sheep's wool	1000	1084	1163	
Beaver's fur	1000	1072	1125	
The fur of a Russian hare	1000	1065	1115	
Eider down	1000	1067	1112	
Silk	Raw, single thread	1000	1057	1107
	Ravelings of white taffety	1000	1054	1103
Linen	Fine lint	1000	1046	1102
	Ravelings of fine linen	1000	1044	1082
Cotton wool	1000	1043	1089	
Silver wire, very fine, gilt, and flatted, being the ravelings of gold lace	1000	1000	1000	

N. B. The weight made use of in these experiments was that of Cologne, the *parts* or least divisions being $= \frac{1}{65536}$ part of a mark, consequently 1000 of these *parts* make about $52\frac{1}{2}$ grains Troy.

I did not add the silver wire to the bodies above mentioned from any idea that that substance could possibly imbibe moisture from the atmosphere; but I was willing to see whether a metal, placed in air saturated with water, is not capable of receiving a small addition of weight from the moisture attracted by it, and attached to its surface; from the result of the experiment, however, it should seem that no such attraction subsists between the metal I made use of, and the watery vapour dissolved in air.

I was totally mistaken in my conjectures relative to the results of the experiments with the other substances. As linen is known to attract water with so much avidity; and as, on the contrary, wool, hair, feathers, and other like animal substances, are made wet with so much difficulty, I had little doubt but that linen would be found to attract moisture from the atmosphere with much greater force than any of those substances; and that, under similar circumstances, it would be found to contain much more water: and I was much confirmed in this opinion upon recollecting the great difference in the apparent dampness of linen and of woollen clothes, when they are both exposed to the same atmosphere. But these experiments have convinced me, that all my speculations were founded upon erroneous principles.

It should seem, that those bodies which are the most easily wet, or which receive water, in its unelastic form, with the greatest ease, are not those which in all cases attract the watery vapour dissolved in the air with the greatest force.

Perhaps

Perhaps the apparent dampness of linen, to the touch, arises more from the ease with which that substance parts with the water it contains, than from the quantity of water it actually holds: in the same manner as a body appears hot to the touch, in consequence of its parting freely with its heat, while another body, which is actually at the same temperature, but which withholds its heat with greater obstinacy, affects the sense of feeling much less violently.

It is well known, that woollen clothes, such as flannels, &c. worn next the skin, greatly promote insensible perspiration. May not this arise principally from the strong attraction which subsists between wool and the watery vapour which is continually issuing from the human body?

That it does not depend entirely upon the warmth of that covering, is clear; for the same degree of warmth, produced by wearing more cloathing of a different kind, does not produce the same effect.

The perspiration of the human body being absorbed by a covering of flannel, it is immediately distributed through the whole thickness of that substance, and by that means exposed by a very large surface to be carried off by the atmosphere; and the loss of this watery vapour, which the flannel sustains on the one side, by evaporation, being immediately restored from the other, in consequence of the strong attraction between the flannel and this vapour, the pores of the skin are disencumbered, and they are continually surrounded by a dry, warm, and salubrious atmosphere.

I am astonished, that the custom of wearing flannel next the skin should not have prevailed more universally. I am confident it would prevent a multitude of diseases; and I know of

no greater luxury than the comfortable sensation which arises from wearing it, especially after one is a little accustomed to it.

It is a mistaken notion, that it is too warm a cloathing for summer. I have worn it in the hottest climates, and in all seasons of the year, and never found the least inconvenience from it. It is the warm bath of a perspiration confined by a linen shirt, wet with sweat, which renders the summer heats of southern climates so insupportable; but flannel promotes perspiration, and favours its evaporation; and evaporation, as is well known, produces positive cold.

I first began to wear flannel, not from any knowledge which I had of its properties, but merely upon the recommendation of a very able physician (Sir RICHARD JEBB); and when I began the experiments of which I have here given an account, I little thought of discovering the physical cause of the good effects which I had experienced from it; nor had I the most distant idea of mentioning the circumstance. I shall be happy, however, if what I have said, or done, upon the subject, should induce others to make a trial of what I have so long experienced with the greatest advantage, and which, I am confident, they will find to contribute greatly to health, and consequently to all the other comforts and enjoyments of life.

I shall then think these experiments, trifling as they may appear, by far the most fortunate, and the most important ones I have ever made.

With regard to the original object of these experiments, the discovery of the relation which I thought might possibly subsist between the warmth of the substances in question, when made use of as cloathing, and their powers of attracting

moisture from the atmosphere; or, in other words, between the quantities of water they contain, and their conducting powers with regard to heat; I could not find that these properties depended in any manner upon, or were in any way connected with, each other.

The result of my experiments upon the conducting powers of these substances, I reserve for a future communication.

